Response to reviewer #1 to «A model for the spatial distribution of snow water equivalent parameterised from the spatial variability of precipitation" by T. Skaugen and I.H. Weltzien.

Let us first express our gratitude for the reviewers who spend their precious time securing the quality of our research, it is very appreciated.

We have tried to break down the general comments into separate statements and will provide a response and a suggestion of correction to each of these.

General comments:

1.The context of the research, however, is not clearly formulated in the introduction.

Response: We agree that, at present, the introduction could be more focused. What we want to bring across is 1) that hydrological models has too many free parameters which constitutes a problem for making predictions in ungauged basins and for a changed climate. In addition 2) we want to demonstrate that that the proposed algorithm for the spatial frequency distribution of SWE which is not calibrated against streamflow is a good alternative.
Change: We propose to restructure and shorten the introduction in order to focus more on the two points above. We will drop the degree-day melt model as an example of a calibrated model (p.3,l.17-p.4,l.3) since it probably just confuses the issues and instead use the calibrated log-normal spatial distribution of SWE since it is directly relevant. Furthermore, the discussion of large sample hydrology (p5, l11-p.6,l3) is, perhaps not very relevant and can be dropped or alternatively moved to the discussion. The detailed description of SD_LN (p.6,l.4-18) can be moved to a subsection of the methods.

2. The basic assumptions and previous literature on the use of PDF of SWE is not clearly presented, nor the difference to SWE modelling based on simple degree-day or more sophisticated physically based snow modelling.

Response: In the introduction we emphasize the importance of a realistically simulated PDF of SWE (p.4, l.19-p.5, l.3) and section 2 "Methods" (p.8,l.1-14) starts with a review of the many statistical models used for the PDF of SWE. Furthermore, the topic is revisited in the discussion (p.25, l.13-p.26, l.15).
In this study we do not consider the modelling aspects of snowmelt, only the spatial distribution (PDF) of SWE. The degree-day model is a snowmelt model, and by "more sophisticated physically based snow models" we suppose R#1 refers to point models like SNOWPACK and CROCUS, which are not used for catchment modelling and are hence not relevant for this study.
Change: The review on PDF models for SWE in section 2 is more suitably placed in the new, more focused introduction. It is outside the scope of the paper to also discuss snowmelt and point models.

3. I would suggest to clearly outline the approach and also present literature which combines such statistical models with rainfall runoff modeling in the past. In the methodology some basic outline would be also useful (e.g. some schematics how the snow accumulation and melt is modelled by the approach).

Response: Both reviewers R#1 and R#2 have comments regarding the structure of the paper, and we can understand that the paper would improve with the restructuring of especially the introduction and the methods (Section 2).
Change: In a restructured and more focused introduction, the approach of this study will be more clearly outlined. The methods section will have an introduction, an overview, where the different steps for estimating the spatial PDF of SWE is outlined. The procedure for snowmelt is described in section 2.5 (p.16, l18-21)

4. Moreover the results might be elaborated in more thorough way (including figures). I agree that using a large sample of basins is important, but the results do not show much of the value of such large dataset. It will be

interesting, for example, to stratify the basins in the figures according mean elevation, size, or some other characteristics to show some more information than just the efficiency.

Response: Again, this comment is common for both R#1 and R#2, and we think this is a good point.
Change: We will describe the results on runoff, SWE and SCA stratifying the catchments as suggested. In a preliminary analysis we found that for 26 out of the 71 catchments (37 %) the annual increase in SWE using SD_LN was more than 5mm/year. For SD_G only 7 out of 71 catchments (10 %) exhibited such a behaviour. This feature greatly influences the duration of the snow season and the mean annual duration of the snow season was 347 days using SD_LN and 228 days using SD_G. The positive trend in SWE using SD_LN could not be associated with either catchment size, mean elevation, of the catchments, or any other catchment characteristic such as fraction of lakes, bogs, forest and bare rock. The trend was, however, significant positively correlated to the parameter θ_{CV}, which is, in turn, significantly positively correlated to the KGE skill score for DDD_LN. To summarize, the positive trend in SWE using SD_LN is not associated with physiographic characteristics but is due to unrealistically high values of θ_{CV} which favours the simulation of runoff at the cost of unrealistic simulations of SWE. For the simulation of SCA, the RMSE using SD_G was not significantly correlated to any catchment characteristics. The RMSE using SD_LN was found to be significant negatively correlated only to the mean elevation of the catchments implying that SCA was better simulated for high elevation catchments using SD_LN. This is consistent with our discussion that SD_LN is better at simulating the initial development of snow-free areas. The mean absolute error (MAE) of the simulation of SCA showed that, in general, for all of Norway, there is an overestimation of SCA (more so for SD_LN (6%) than SD_G (3%)). There is a regional pattern, however, in that the underestimations for both methods (SD_G and SD_LN) are found mostly for the catchments of southern west-coast of Norway. The MAE for SCA is significant positively correlated for catchment size, and many small catchments are found in this area. In the revised version of the paper we will elaborate on the results and their link to catchment characteristics as suggested above.

5. It is not very clear, why the improved snow simulations do not result in better runoff simulations. Some more evaluations will be interesting here.

Response: Again, this comment is common for both R#1 and R#2, and ideally one would expect improved runoff simulations when the snow is better simulated. The failure to do so, however, is not an uncommon feature for hydrological models with many free calibration parameters. In Parajka et al. (2007) they found that when the hydrological model was calibrated against snowcover data in addition to runoff, snow simulations got better, but runoff simulations deteriorated. In our own example shown in Figure 10, SD_LN performs best with respect to runoff simulations when unrealistic snow is simulated, a clear example of a model that works well with respect to runoff, but not for the right reasons. The reason for such a behavior is probably due to inadequate model structures. When the parameter for the spatial distribution of SWE in SD_LN is allowed to be optimized against runoff without physical constraints, unreasonable values for the parameter may be the result. If, however, the snow distribution is “forced” to behave realistically, given the (inadequate) model structure, the runoff simulations deteriorate quite substantially. When SD_G is used, however, we get both reasonably good runoff and snow simulations.

Change: We will elaborate on this in the discussion section with arguments used above.

Specific comments:

1) Abstract: The applied methodology and model concept is not clearly presented (the abbreviations SD_G, LN are not very intuitive). The period used for analyses is missing

Response: Clearly the abbreviations should be spelled out. We do find it difficult, however, to see major points where improvements on the presentation can be made. The main point is that one method is calibrated against runoff and the other method is not. There are not much room for going into details on the method.

Change: We will spell out the abbreviations, include the period used for analysis and try to make the outline more clear.

2) Introduction: This part does not have a clear story. It mixes different topics, but does not clearly outline the research problematic and does not clearly show what the results of previous studies are. The meaning and basics behind the PDF modelling needs to be introduced on lower technical level.

Response: This is a similar comment to the first general comment and we agree.
Change: see response and change to first general comment.

3) Modeling: It is not clear whether the results show the calibration or validation period.

Response: That is true. The models were calibrated on data from 19858(1.9)-2000(31.8) and validated on data from 2000(1.9) -2014(31.12)
Change: This information will be inserted at the appropriate place (in section 2.5)

4) Snow cover area results. It will be interesting also to see the model performance in terms of snow cover duration.

Response:Yes, and this comment is in line with that of R#2 for Page 20 line 2: An analysis of snow cover duration will reveal how many catchments that suffers from "snow-towers" using SD_LN
Change: We will analyse the snowcover duration using SD_G and SD_LN, and answer both this comment and that of R#2.

5) Please check references. They are not always complete and consistent.

Response: Yes
Change: We did not find any references that where in the text and not in the reference list and vice versa, but we will edit the references in the text (correct ordering) and check the format in the reference list.

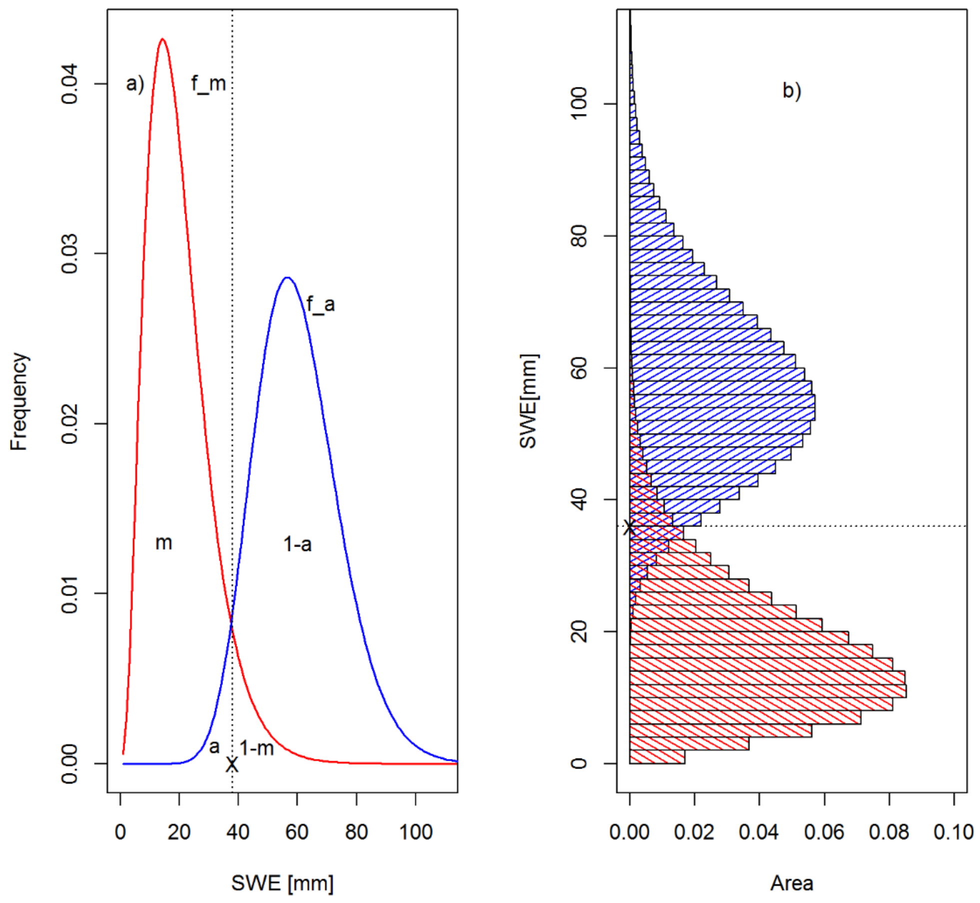
6) Table2: Which period?

Response: Sorry, an omission.
Change: Correct period (2000-2014) will be inserted

7) Fig.2: A schematic would be important to understand the method, however, here it is not clear. From the Figure and caption, the meaning of a,s, F_s, etc is not clear.

Response: We understand that this might be hard to grasp.
Change: We will elaborate further on the explanation in the text and on the figure. We suggest the following:

In the model for estimating the reduction of SCA after a melting event, we assume that areas with smallest SWE are the first to become snow free since the energy requirements for melting a column of snow is proportional to the height of snow (Dingman, 2002). Figure 2 a) shows the PDFs of melt (f_m, red) and accumulation (f_a, blue). In Figure 2 b) we have plotted the integral of the PDFs for successive intervals of SWE, so each horizontal bar represents a fractional area (see the x-axis) covered by SWE or melt values. The horizontal bars for each integrated PDF sum up to unity, i.e. the entire area covered by snow. The figures illustrates that melt values less than X cover a large area (the integral of f_m up to X, called m, $\int_0^X f_m = m$ in the Figure 2a) and much larger than the area of SWE values less than X (the integral of f_a up to X,called a, $\int_0^X f_a = a$ in Figure 2a). Consequently, the fractional area of SWE values less than X, a, becomes snow free after the melting event. In addition, there are melt values higher than X that reduce the coverage of corresponding SWE values. The sum of these bars adds up to $1 - m$, and equals the integral $\int_X^\infty f_m = 1 - m$. In total, the snow free area after a melting event is $a + 1 - m$ and is seen in Figure 2b) as the sum of the cross-hatched bars.



New Figure 2.